IN THE UNITED STATES DISTRICT COURT
FOR THE NORTHERN DISTRICT OF OKLAHOMA

STATE OF OKLAHOMA,	:	
	:	
Plaintiff,	:	
	:	
v.	:	**Case No. 4:05-CV-329-GKF-PJC**
	:	
TYSON FOODS, INC., *et al.*,	:	
	:	
Defendants.	:	

STATE OF OKLAHOMA'S MOTION IN LIMINE TO EXCLUDE PORTIONS OF DEFENDANTS' "EXPERT REPORT OF WILLIAM H. DESVOUSGES AND GORDON C. RAUSSER" AND RELATED TESTIMONY WITH INTEGRATED BRIEF IN SUPPORT

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Plaintiff, the State of Oklahoma ("the State"), pursuant to Federal Rules of Evidence 104 and 702 and *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 U.S. 579 (1993), respectfully moves this Court for an order *in limine* excluding portions of Defendants' damages report entitled "Expert Report of William H. Desvousges, Ph.D. and Gordon C. Rausser, Ph.D.," dated March 31, 2009 ("D/R Report") and precluding the related expert testimony of Defendants' testifying witnesses Drs. Desvousges and Rausser (collectively, "D/R").

I. Background

A. The CV Study

As the Court is aware, the State's team of internationally known experts in environmental economics, natural resource damage assessments, and survey methodology, led by Stratus Consulting, developed a survey that was administered to a large sample of Oklahoma residents. This work culminated in two expert reports on damages served on January 5, 2009. The first is entitled "Natural Resource Damages Associated with Aesthetic and Ecosystem Injuries to Oklahoma's Illinois River System and Tenkiller Lake" (hereinafter "CV Report"). This report was authored by Dr. Richard Bishop, Mr. David Chapman, Dr. Michael Hanemann, Dr. Barbara Kanninen, Dr. Jon Krosnick, Dr. Edward Morey, and Dr. Roger Tourangeau. Using the contingent valuation methodology, the CV Report provides a measure of the monetary value placed on aesthetic and ecosystem injuries to the Illinois River system and Tenkiller Lake (from 2009 to 2058 for the Illinois River system and from 2009 to 2068 for Tenkiller Lake). The CV study was undertaken within the framework of natural resource damage assessment ("NRDA") set forth in the Department of the Interior's NRDA regulations ("CV Study"). (*See* Dkt. #1853-5, CV Report, p. ES-1.) The CV Study developed a conservative measure of these damages, by estimating the mean willingness-to-pay for a hypothetical alum treatment program that would

return the flow of services from the Illinois River system and Tenkiller Lake to their 1960 condition 40 years sooner than without the program (hereinafter "the scenario"). (*Id.*, p. 1-9.) As stated in the CV Report, the scenario "allowed respondents to make a choice about a well-defined, realistic tradeoff. Either they could greatly reduce the injury and pay the tax for the alum treatments or accept the natural recovery without the alum treatment and use their money for other purposes." (*Id.*, p. 1-7.) The tradeoff respondents valued was the accelerated improvement in the aesthetic and ecosystem conditions of the Illinois River System and Tenkiller Lake. The second report, addressing past damages, is entitled "Natural Resource Damages Associated with Past Aesthetic and Ecosystem Injuries to Oklahoma's Illinois River System and Tenkiller Lake" ("Past Damages Report").

B. Desvousges/Rausser Report

Responding to the State's CV Report and Past Damages Report, Defendants produced, among others, the D/R Report. (Ex. A, D/R Report; Ex. B, Errata.) Distilled into four parts, the D/R Report: (1) presents a regression model analyzing visitation at various Oklahoma lakes (Chapter 2); (2) presents a property valuation model comparing real estate property values around Lakes Tenkiller and Eufaula (Chapter 3); (3) provides a separate critique of the CV Study/Report (Chapters 4-6); and (4) critiques the Past Damages Report (Chapter 7).[1]

II. Legal Standard

The standard governing the admissibility of expert testimony is well-settled. *Ralston v. Smith & Nephew Richards, Inc.*, 275 F.3d 965, 969 (10th Cir. 2001). As an initial matter, the court must determine whether the expert is qualified by "knowledge, skill, experience, training,

[1] Defendants have indicated they may call at trial Dr. Desvousges to testify as to all matters in the D/R Report and Dr. Rausser to testify as to the statistical and econometric analyses and results set forth in the Report. (*See* Dkt. #2242-5, 3/31/09 Letter from T. Hill to R. Garren.)

or education" to render an opinion. Fed. R. Evid. 702. The issue is "whether those qualifications provide a foundation for a witness to answer a specific question." *In re Williams Sec. Litig.*, 496 F. Supp. 2d 1195, 1232 (N.D. Okla. 2007) (internal quotation marks omitted). Next, the court must ensure that the scientific testimony being offered is "not only relevant, but reliable." *See Daubert*, 509 U.S. at 589. "To be reliable under *Daubert*, an expert's scientific testimony must be based on scientific knowledge . . . ." *Dodge v. Cotter Corp.*, 328 F.3d 1212, 1222 (10th Cir. 2003). The term "scientific" "implies a grounding in the methods and procedures of science." *Daubert*, 509 U.S. at 590. Likewise, "the word knowledge connotes more than subjective belief or unsupported speculation." *Id.* "[I]n order to qualify as 'scientific knowledge,' an inference or assertion must be derived by the scientific method. Proposed testimony must be supported by appropriate validation – i.e., 'good grounds,' based on what is known." *Id.*

The Supreme Court has set forth four non-exclusive factors that a court may consider in making its reliability determination: (1) whether the theory or technique can be (and has been) tested; (2) whether the theory or technique has been subjected to peer review and publication; (3) the known or potential rate of error and the existence and maintenance of standards controlling the technique's operation; and (4) whether the theory or technique has general acceptance in the scientific community. *Id.* at 593-94. The inquiry is "a flexible one," *id.*, but the "focus [of the inquiry]. . . must be solely on principles and methodologies, not on the conclusions that they generate." *Id.* at 595.

Ultimately, "the inquiry of whether expert testimony will assist the trier of fact is essentially a question of relevance." *United States v. Arney*, 248 F.3d 984, 990 (10th Cir. 2001); *accord Smith v. Sears Roebuck & Co.*, No. CIV-04-1271-HE, 2006 WL 687151, at *3 (W.D. Okla. Mar. 17, 2006). Courts should exclude expert testimony if it lacks relevance. *See, e.g.*,

Smith, 2006 WL 687151, at *5 (excluding expert testimony on relevancy grounds); *Arney*, 248 F.3d at 991; *Phillips v. Hillcrest Med. Ctr.*, 244 F.3d 790, 801 (10th Cir. 2001) (same). Exclusion is proper when such proposed testimony is based on a false premise. *See, e.g.*, *Magoffe v. JLG Indus., Inc.*, No. CIV 06-0973, 2008 WL 2967653, at *18 (D.N.M. May 7, 2008). Furthermore, "[o]pinions derived from erroneous data are appropriately excluded." *United States v. City of Miami*, 115 F.3d 870, 873 (11th Cir. 1997); *see also Slaughter v. S. Talc Co.*, 919 F.2d 304, 307 (5th Cir. 1990).

In sum, *Daubert*'s "objective . . . is to ensure the reliability and relevancy of expert testimony." *Kumho Tire Co. v. Carmichael*, 526 U.S. 137, 152 (1999). The party proffering the testimony bears the burden of establishing its admissibility. *Ralston*, 275 F.3d at 970 n.4.

III. Argument

A. D/R's Recreation Model and Any Related Testimony Should Be Excluded

In Chapter 2, D/R estimated a regression model "to understand the factors that affect visitation to COE [Corps of Engineers] lakes in Oklahoma." (Ex. A, p. 17.) "To evaluate the potential effect of water quality on visitation at COE lakes, [D/R] used the average water clarity of the lake," i.e., the "meanclarity" variable. (*Id.*, p. 18.) Based on their model, D/R opine: "Our analysis indicates that none of the indicators for water clarity were found to significantly predict visitation." (*Id.*) Thus, they conclude that "recreation at Tenkiller Lake has not been impacted by changes in water quality and . . . recreators have not experienced any potential losses from alleged injuries attributable to increased phosphorous loadings" from poultry litter. (*Id.*)

D/R's regression model contains at least three data errors. Correcting for any of these reverses the statistical significance of the "meanclarity" variable, invalidating D/R's conclusion that "none of the indicators for water clarity [i.e., the meanclarity variable] were found to significantly predict visitation" (Ex. A, p. 18). (Ex. C, Kanninen Decl. ¶¶ 6-17.) Thus, D/R's

model and related testimony should be excluded as unreliable under Rule 702 and *Daubert*.

1. *D/R's Miscoding of Lake Tenkiller's 2007 Visitation Number*

The first error in D/R's model is a miscoded visitation number on Tenkiller Lake for 2007, using **294,047** instead of **2,924,047**, due to a dropped digit.[2] (Ex. C, Kanninen Decl. ¶ 11.) The data file on which the regression was run[3] (Exhibit F) shows that the incorrect number was used in the model, and Desvousges acknowledges the error.[4] (Ex. E at 171:10-25, 172:10-12; *see also* Ex. C, Decl. ¶ 11.) When D/R's model is re-run with this single error corrected, the estimated influence of "meanclarity," D/R's indicator of water clarity, is significantly positive. (Ex. C, ¶¶ 8, 10-12.) This result directly contradicts D/R's opinion that water clarity has no influence on lake visitation, and reverses that finding. (*Id.* ¶¶ 16-17.)

2. *D/R's Omission of Broken Bow Data*

The second error in D/R's model is the omission of data for Broken Bow Lake, despite D/R's claim that they used "the 22 COE lakes in Oklahoma that have data on lake levels" (Ex. A, p. 17), which would include Broken Bow Lake. (Ex. C, Kanninen Decl. ¶ 14.) In fact, D/R use only 20 lakes in their model. Exhibit F, which is "the data file that the regression was run on"

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[2] Pages 2-4 of Exhibit D is a spreadsheet generated by Desvousges' staff called "data," which reflects the data corresponding to all the variables used in the model. (Ex. D, pp. 2-4; Ex. E, Desvousges Depo Tr. at 157:18-158:12, 159:9-24.) With regard to the visitation variable, the spreadsheet was intended to capture, under the column "visits," the lake visitation data provided by the COE on page 1 of Exhibit D. (Ex. E at 167:13-168:1.) Page 1 shows a visitation number of **2,924,047** for Tenkiller Lake for 2007. (Ex. D, p. 1.) As is clear, however, the number of visits for the year 2007 that was entered in the D/R data file for Tenkiller Lake (Lake #23) is **294,047**, instead of 2,924,047. (*Compare id.*, p. 1 *with id.*, p. 4; *see also* Ex. E at 169:10-25.) Desvousges has acknowledged this error. (Ex. E at 169:14-15.)

[3] A member of Desvousges' staff entered the information into the data file for the regression model. (Ex. E at 157:18-158:12, 171:10-172:2.) Desvousges neither personally ran the model nor reviewed the code used to run the model, and he could not say who reviewed the code, if anyone. (*Id.* at 155:2-5, 156:24-157:12.)

[4] Desvousges did not know how this error impacted the significance of the meanclarity variable. (*See* Ex. E at 170:19-171:9.)

(Ex. E at 171:10-25), reflects lake data for 20 lakes from 2000-2007 (i.e., Lakes 1, 2, 4, 6-15, 17, 19, 21-23, 25, and 27). (*See also id.* at 162:10-163:25; Ex. D.) Broken Bow Lake (Lake #3) was not included in the model. (Ex. C, ¶ 14.) Re-estimating D/R's regression model with the data for Broken Bow Lake establishes that the meanclarity variable *is* statistically significant (i.e., lake visitation is a function of water clarity). (*Id.* ¶¶ 15.) This result negates D/R's claim that lake visitation is *not* a function of water clarity. (*Id.* ¶¶ 16-17.)

3. *D/R's Miscoding of Lake Fort Supply's Lake Depth*

The third error in D/R's regression model is the miscoded lake depth for Fort Supply Lake. (Ex. C, Kanninen Decl. ¶ 13.) "Lakedepth" was one of the independent variables used in the D/R regression model. (Ex. A, p. 18, Table 2.1; Ex. E at 172:17-20.) D/R defined the "lakedepth" variable as "[n]ormal water elevation as indicated by the Corps of Engineers (feet)." (Ex. A, p. 18; *see also* Ex. E at 173:4-174:7.) As shown on Exhibit F, the lake depth used in the model for Lake Fort Supply (Lake #9) was zero feet. (*See also* Ex. C, ¶ 13; Ex. E at 174:14-175:1.) Lake Fort Supply's COE-reported lake depth, however, is 2,004 feet.[5] (Ex. G; *see also* Ex. C, ¶ 13; Ex. E at 175:2-6.) Desvousges could not explain why the data file on which the regression was run reflects a lake depth of zero, and he did not know whether this error impacted the significance of the meanclarity variable. (Ex. E at 175:7-14.) The result of correcting this error in their model is that "meanclarity" – D/R's indicator of water clarity – is statistically significant, again negating D/R's claim that it is not and that lake visitation is *not* a function of water clarity. (Ex. C, Kanninen Decl. ¶¶ 13, 16-17.)

Finally, the attached Declaration of Dr. Kanninen also provides estimation results for any and all combinations of error corrections, including all three corrections at once. (*Id.* ¶¶ 11-16.)

[5] This variable was mis-named by D/R; the "lakedepth" data they use correspond to the "normal elevation at the top of the conservation pool," not actual lake depth. (Ex. C, ¶ 13 n.1.)

When any, all, or any combination of the foregoing three errors is corrected, and D/R's regression model is re-run, the "meanclarity" variable is found to be positive and statistically significant, meaning that water clarity does impact lake visitation, directly contradicting D/R's claim that it does not. (*Id.* ¶¶ 16-17.) Where, as here, the expert's opinions are not supported by the data upon which the expert relies, a trial court need not admit the expert's testimony. *GE v. Joiner*, 522 U.S. 136, 146 (1997) (explaining that courts should not "admit opinion evidence which is connected to existing data only by the *ipse dixit* of the expert"). Because D/R's model is not supported by the data, the Court should exclude the opinions in Chapter 2 as unreliable.

B. D/R's "Hedonic" Model and Related Testimony Should Be Excluded

In Chapter 3, D/R use the hedonic property value approach to compare property values for homes near Lakes Tenkiller and Eufaula to "test whether the value of properties located near Tenkiller Lake were affected by phosphorous."[6] (Ex. A, p. 21.) D/R propound two hypotheses:

> Hypothesis (1): "Other things being equal, a home located on or near a lake that is aesthetically impaired [Tenkiller] would be expected to have a lower price than a similar house located [on or near a] lake that is not impaired [Eufaula]." (*Id.*, p. 22.)
>
> Hypothesis (2): "[E]ven if Eufaula Lake and Tenkiller Lake were not comparable lakes, i.e., there are characteristics that differentiate the two lakes, we would expect that as the alleged phosphorus problem [at Tenkiller Lake] worsened over time, the relative effect on home prices would be negative." (*Id.*)

D/R test these hypotheses using data from 1995 to 2008 from a sample of sales of single family homes located within one mile of Tenkiller Lake (Cherokee and Sequoyah Counties) and northwest Lake Eufaula (McIntosh County). (*Id.*, pp. 23-24.) D/R assert that their data reject the

[6] "The hedonic method for non-market valuation relies on market transactions for [] differentiated goods to determine the value of key underlying characteristics." (Ex. H, p. 331.) Differentiated goods are "products whose characteristics vary in such a way that there are distinct product varieties even though the commodity [e.g., houses] is sold in one market....[T]he hedonic method is an 'indirect' valuation method in which we do not observe the value consumers have for the characteristics directly, but infer it from observable market transactions." (*Id.*)

two hypotheses and opine that "there is no evidence, based on actual market transactions, that water quality has negatively impacted the valuation of single family homes on Tenkiller Lake."[7] (*Id.*, p. 25.) Because D/R's analysis suffers from fundamental methodological flaws, it is of no assistance to the trier of fact and must be excluded under *Daubert*.

1. D/R's Hypothesis (1)

First, D/R's test of hypothesis (1) – which expressly requires "other things being equal" – is meaningless because it fails to control for many *other* factors besides water quality that could impact home prices near Tenkiller Lake compared to those near Lake Eufaula. Hence, other things are *not* equal in D/R's test. Thus, D/R's model to test hypothesis (1) provides no information that could assist the trier of fact.

In this regard, D/R estimated a regression model of the sale price of single family homes, acknowledging that "[t]here is wide variation of attributes of the single family home. Hence, it is necessary that we control for these various attributes that are expected to affect the transaction price of single family homes located near each of the two lakes." (Ex. A, p. 25.) D/R's hedonic model only controlled, however, for house-specific characteristics: square footage, number of bedrooms and bathrooms, condition of building, age of house at sale date, type of air/ventilation, and year of sale. (*Id.*) D/R test their hypothesis (1) by adding, as an explanatory variable, a dummy variable for Tenkiller Lake, which they claim represents the difference in water quality between the two lakes. As described below, the use of a dummy variable to represent water quality is not supported by the peer-reviewed literature and, in practice, is subject to a confounding effect that renders any interpretation of the effect of the variable to be arbitrary.

[7] As an initial matter, it is possible for the injuries in the Illinois River System and Tenkiller Lake to reduce the well-being of the people of Oklahoma in ways that are *not* reflected in the price of homes sold in Cherokee and Sequoyah Counties.

D/R assume in their model that, after controlling for the structural (house-specific) characteristics, any systematic difference in the prices of homes sold during the study period near Lake Tenkiller versus those sold near Northwest Eufaula Lake must reflect *only the difference in water quality* at Tenkiller Lake compared to Lake Eufaula, and nothing else. The assumption is neither plausible nor reasonable, and it does not find support in the peer-reviewed literature.

Researchers performing a hedonic model generally control for additional factors that may affect price besides structural characteristics.[8] In a review of recent literature on the hedonic approach, Professor Laura Taylor discusses the variables to be included in a hedonic regression:

> In general, most property value studies include three types of characteristics: (1) the house and the lot, (2) features of the neighborhood such as the quality of the school district, the level of crime, and the environmental health and (3) the property's location such as its proximity to a recreation area or an employment center.

(Ex. H, Laura O. Taylor, *The Hedonic Method,* A Primer on Nonmarket Valuation 331, 344-45 (Eds. Champ, Boyle, Brown 2003); *id.* ("Researchers must use their knowledge of the market to determine what characteristics are relevant for determining price in their market."). Professor Taylor notes that a researcher "should have a good understanding of the market."[9] *Id.* at 342. Here, D/R's hedonic model does not follow the literature on the hedonic approach. And it is evident that, contrary to the standards governing researchers in this regard, D/R have little understanding of the housing markets around Lake Tenkiller and Lake Eufaula.

D/R did include some house characteristics in their analysis, as Taylor recommends. However, D/R did *not* include any characteristics of neighborhoods or of locations and, therefore, did not follow the procedure outlined by Taylor. This invalidates D/R's analyses.

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[8] As explained below, Rausser has done so in other studies, but failed to do so here.

[9] When asked whether he agreed that, when estimating a hedonic regression model of house prices, the researcher should have a good understanding of the housing market covered by the data, Rausser said "No." (Ex. I at 53:14-18.)

They wish to ascribe all observed differences between the two areas to differences in the lake water quality. In fact, however, many other differences also exist and are confounded with differences in water quality, therefore masking water quality's true influence on property values.

A few of the other differences that mask the impact of water quality include: (1) a casino near Lake Eufaula (Ex. K); (2) Lake Eufaula has more than four times the amount of shoreline as Lake Tenkiller (Ex. J, 2006 Fast Facts for Lakes Tenkiller and Eufaula.); (3) Eufaula has more than five times the amount of surface acreage as Tenkiller (*id.*); (4) Eufaula has fewer marinas than does Tenkiller (*id.*); and (5) more than 250 housing developments near Eufaula, but many fewer homes apparently near Tenkiller (Ex. L). In order to properly isolate the impact of water quality differences on housing price differences between the two lakes, D/R would have to have taken into account these and many other differences between locations. But they did not do so.[10]

Although the number of other available lakeside homes in an area can influence the value of any given lakeside home, and although the housing stock in a market is likely to influence the number of homes for sale at any given time and hence the price, Rausser acknowledged that he did not take housing stock into account in his comparison of the two lakes. He said, "I haven't analyzed the stock. I've only analyzed the flow." (Ex. I at 81:18-82:17.)

Furthermore, to validly compare housing prices near Lake Tenkiller to those near Lake Eufaula, the lakes would both need to be within a single housing market. However, D/R provided no evidence that this is the case. In fact, when asked whether Cherokee and Sequoyah Counties (which contain Lake Tenkiller) are in the same housing market as McIntosh County (which contains northwest Lake Eufaula), Rausser answered: "That's not a question that I've analyzed. I haven't visited that question and have no opinion in that regard." (*Id.* at 68:25-69:7.)

[10] Rausser conceded that other differences such as these would bias the apparent impact of the dummy variable for Lake Tenkiller in his analysis. (Ex. I at 75:23-76:7.)

Rausser nonetheless claimed during his deposition that the market conditions (what he called "external factors") near Lake Tenkiller "are basically the same as those that exist for Lake Eufaula." (*Id.* at 62:1-4.) Although he claimed to have analyzed the demographics and economic conditions in the two areas, Rausser did not report any such analysis (*id.* at 62:20-63:1), and no evidence exists documenting that any such analysis was done. Indeed, to the contrary, available data suggest that "external factors" do in fact differ importantly between the two communities. For example, data from the U.S. Census Bureau show differences in demographics between Cherokee and Sequoyah Counties on the one hand versus McIntosh County on the other, including differences in populations, population densities, age, and race. (*See* Exs. N-P, U.S. Census Bureau, State & County Quick Facts: Cherokee County, Sequoyah County, and McIntosh County.) Such differences invalidate claims that the two lakes are contained within the same housing market.

Rausser's own publications illustrate that he is aware of how to conduct such comparisons properly. For example, in a peer-reviewed journal article, Rausser used the hedonic property value approach to measure the impact of a hazardous waste site in Dallas County, Texas on property values. (Ex. M, J. McCluskey & G. Rausser, *Stigmatized Asset Value: Is It Temporary or Long-Term?*, The Review of Economics and Statistics (May 2003), 85(2):276-285.) In that study, unlike here, Rausser properly followed the methodology described by Professor Taylor, by including variables from all three categories of necessary property value predictors.[11] *Id.* at 279.

[11] To measure neighborhood characteristics, Rausser used three demographic variables, namely, of the census tract, (1) percentage below the poverty line, (2) percentage who are Hispanic, and (3) percentage who are African-American. *Id.* No demographic variables were used in D/R's hedonic model of Lake Tenkiller. (Ex. I at 94:3-8 (Rausser testified that he did not know percentages of the census track below the poverty line for Lake Eufaula and Lake

Moreover, D/R's hedonic model does not follow the same approach as the three studies in the literature that D/R cite (Ex. A, p. 22). Those studies used a hedonic property value regression to assess the economic impact of water quality in a lake. These three studies followed the approach outlined by Professor Taylor and (unlike D/R) included variables representing neighborhood, location, and house characteristics. (Ex. Q at 287, 290; Ex. R at 42; Ex. S at 803.) In particular, those studies employed several key variables lacking from D/R's analysis, including a measure of water quality. (The Michael et al. (2000) study and Gibbs et al. (2002) study used *water clarity*, while the Poor et al. (2007) study used *total suspended solids* and *dissolved inorganic nitrogen.* (Ex. Q at 290; Ex. R at 42; Ex. S at 803.)) D/R's approach of using a single dummy variable for one lake while controlling for no lake or area characteristics does not allow them to attribute all the difference between the lakes to water quality alone; no other study in the peer-reviewed literature has done the overly simplistic analysis that D/R did.

An important feature of all hedonic studies is that sale properties that are used to estimate the models include variation, more that two levels of the environmental variable of interest; water clarity here. Both the Michael et al. (2000) study (Ex. Q) and the Gibbs et al. (2002) study (Ex. R) include sales of properties from multiple lakes with each lake having a different level of water clarity. The Poor et al. (2007) study (Ex. S) uses property sales from multiple locations on the Chesapeake Bay with different measures total suspended solids and dissolved inorganic nitrogen. Thus, these studies do not depend on all other factors being equal, but actually investigate how sale prices vary with changes in the environmental variable of interest. The failure to include property sales with many lakes with different water quality and the failure to

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Tenkiller regions).) Yet poverty levels are quite different in the two regions. (*Compare* Exs. N-P (U.S. Census data).) Also, although Rausser controlled for distance from the Galleria mall in his Dallas study (Ex. M at 279), D/R's model did not use any similar location variable (Ex. K).

include water quality as an explanatory variable in their model again results in D/R departing from the standard practice in the literature.

In short, D/R's hedonic model fails to conform to the standard practice in the literature to investigate market effects of impaired water quality on residential property values. Specifically, D/R failed to control for numerous factors besides water quality that would mask the impact of water quality differences between Tenkiller Lake and Lake Eufaula on property values in those two areas. Therefore, D/R's hedonic model and any related testimony should be excluded as unreliable under Rule 702 and *Daubert*.

2. D/R's Hypothesis (2)

D/R next hypothesize that even if Lakes Tenkiller and Eufaula "were not comparable lakes, i.e., there are characteristics that differentiate the two lakes, we would expect that as the alleged phosphorus problem [at Tenkiller Lake] worsened over time, the relative effect on home prices would be negative." (Ex. A, p. 22.) This hypothesis (using house prices from 1995 to 2008) rests on the premise that water quality in Tenkiller Lake was growing worse during this period (as opposed to staying in roughly the same injured position). At a minimum, to test this hypothesis requires the presentation of positive evidence that characteristics of quality noticeable by homeowners, and salient to them, were *growing worse* at Tenkiller Lake *compared to* Lake Eufaula *during the period 1995 to 2008*. D/R make no such presentation. (Ex. I at 121:2-23.)

Because D/R's regression model in Chapter 3 does not conform to the peer-reviewed literature on the hedonic property value approach, it is not grounded in sound methodology. Accordingly, the model and all related testimony must be excluded under Rule 702 and *Daubert*.

C. D/R's Opinions Relating To the CV Study's Alum Scenario Should Be Excluded for Lack of Relevance

Throughout Chapter 4 of their Report, D/R challenge the CV Study's validity, claiming

that not all of the alleged scientific facts about real alum treatments were presented in the alum treatment scenario in the survey. (*E.g.*, Ex. A, pp. 33-40, 55, 66.) D/R's opinions in this regard, however, are based on a false premise, namely, that the alum scenario in the survey must be implementable, efficacious, and without collateral impacts. Such matters, however, are irrelevant to the validity of the survey, the survey responses, or the resulting analysis. Accordingly, D/R opinions relating to the information presented to the respondents about the alum scenario should be excluded under Rule 702 and *Daubert*.

A discussion of the CV Study's presentation of the alum scenario is provided in the State's motion *in limine* to exclude Defendants' Connolly-Sullivan-Coale Report. (Dkt. #2242, pp. 2-4, 18-25.) The State incorporates that discussion by reference. That discussion also demonstrates that the validity of the CV survey, the responses thereto, and the resulting analysis are *not* dependent upon the practicality, efficacy, and collateral impacts of the alum scenario presented to the survey respondents.[12] (*Id.*, pp. 18-25.) Moreover, it is standard practice in CV surveys to introduce counterfactual information designed to give respondents a plausible situation within which to consider tradeoffs involved in arriving at their WTP value.[13] To suggest otherwise, as D/R do in critiquing the scenario, is not grounded in CV methodology.

In short, whether the State actually implements an alum treatment program and whether

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[12] In support, the State submitted, among other things, the deposition testimony of Drs. Tourangeau and Krosnick, the State's experts in survey methodology, and Mr. Chapman, project manager for the CV Study, as well as the declarations of Dr. Hanemann, one of the State's economic experts, and Dr. Tourangeau. (*See* Dkt. #2242, pp. 22-24 (deposition testimony); #2242-3 (Hanemann Decl.); #2242-4 (Tourangeau Decl.).)

[13] *See* Robert C. Mitchell & Richard T. Carson, *Using Surveys to Value Public Goods: The Contingent Valuation Method* (1989); Kevin.J. Boyle, *Contingent Valuation in Practice, in* A Primer on Nonmarket Valuation 111, 128-29 (Patricia A. Champ et al., eds., 2003); *see also* Ex. T, NOAA Panel Report, p. 3 ("Typically, CV studies provide respondents with information about a *hypothetical* government program that would reduce the likelihood of a future adverse environmental event such as an oil spill, chemical accident, or the like." [emphasis added]).

such program would be implementable, effective, and cost-effective are considerations that are irrelevant to the validity of the CV Study and resulting CV Report. D/R's opinions regarding these considerations are, therefore, of no assistance to the trier of fact. *Gust v. Jones*, 162 F.3d 587, 594 (10th Cir. 1998). Accordingly, such opinions in Chapter 4 and related testimony are inadmissible under Rule 702 and *Daubert* and should be excluded for lack of relevance.[14]

D. D/R Render a Number of Opinions in Critiquing the CV Study That Should Be Excluded Because They Are Based Solely on Speculation

D/R's critique of the CV Study is riddled with opinions that are based on nothing more than speculation. As such, those opinions, bulleted below, should be excluded. *See, e.g., Jetcraft Corp. v. Flight Safety Int'l*, 16 F.3d 362, 366 (10th Cir. 1993) (expert testimony excluded as professional speculation); *Eastridge Dev. Co. v. Halpert Assocs., Inc.*, 853 F.2d 772, 783 (10th Cir. 1988) (affirming exclusion of proposed expert testimony of speculative nature).

- **The Stratus team "chose to rely solely on the hypothetical CV survey, in part because people's perceptions of water quality for the Illinois River System and Tenkiller Lake, based on both the intercept and telephone surveys, were more favorable than the plaintiffs' experts would have preferred." (Ex. A, p. 1; *see also, e.g., id.*, p. 32.)**

D/R have no basis for stating that the Stratus team chose the CV methodology because they were purportedly unhappy with the results of the preliminary intercept study and telephone survey. D/R do not, and cannot, cite any support for their opinion.

- **"[P]hotos are efficient survey tools. That efficiency is accompanied by the creation of an indelible image in the minds of the respondents. Although the Stratus team claims to use photos that show 'relatively mild' algae growth, the differences are striking. They are so striking, in fact, that it is easy to forget that those conditions, where they exist in the river, are present only during a few months of the year and confined to limited areas." (*Id.*, p. 42.)**

This is pure speculation. D/R cite no data or studies addressing whether images were "indelible"

[14] Moreover, D/R are misleading the Court in trying to imply that the economic tradeoff is intended to value an alum program when, in fact, the economic tradeoff is designed to value an improvement in water quality. Their opinions should be excluded on this additional ground.

or how respondents might have weighed information from the photos against verbal material.

- **"Another critically important but biased facet of the Stratus questionnaire is the statement that asks respondents to assume that the court had decided to impose a ban on the application of poultry litter in the Illinois River watershed. . . . The likely effect of such a statement is to mislead people to think that the court agreed that the application of litter was a serious problem." (Ex. A, p. 42.)**

D/R do not support this opinion (and misrepresentation of the survey) with any empirical data.

- **"[R]espondents formed different assumptions about future phosphorous loads to the river and lake. . . . respondents were valuing different commodities when they hypothetically voted." (Ex. A, p. 47.)**

D/R base this opinion on nothing more than conjecture.

- **"The lack of privacy during a CV vote may result in an upward bias of votes 'for' because the respondents may try to please the interviewer (Tourangeau, Rips, and Rasinski 2000) or may want to appear more socially responsible (Vossler, et al. 2003; Ethier, et al. 2000)." (Ex. A, p. 55.)**

D/R's inference about how the phenomenon of respondents not wanting to incur the interviewer's disapproval might affect responses to CV questions is pure speculation and is not rooted in the cited literature.[15]

- **"The authors of the Stratus Report mention that 58.4% of respondents to the base survey voted 'for' the proposed cleanup program. They fail to report the same measure for the scope survey: which is 42.5% in favor. This result may be driven by the fact that respondents had less faith in the effectiveness of the remediation scenario described in the scope survey and not just the smaller magnitude of the described injury." (Ex. A, p. 65.)**

D/R are only speculating here, and they offer no empirical evidence to support their proposition.

- **"[B]y mingling different timelines and natural resource recovery periods with the difference in geographic dimensions of the two surveys, Stratus left respondents with two very different perceptions about the cost effectiveness of the two programs." (Ex. A, p. 68.)**

Again, D/R offer no empirical support for this proposition.

[15] D/R do not cite any particular section of Roger Tourangeau et al., The Psychology of Survey Response (2000), which is 400 pages and does not discuss CV surveys.

- **"The Stratus survey contains nonresponse bias." (Ex. A, p. 78.)**

D/R do not cite any empirical data to support this opinion, nor do they cite any data to contradict the Stratus team's application of two standard tests for nonresponse bias (which revealed no such "bias") (CV Report, App. F). D/R did not analyze the survey data in arriving at this opinion, and they have no quantitative evidence to support it. (Ex. E, Desvousges Depo Tr. at 92:14-93:20.)

- **"[T]he media coverage has increased awareness of the algae conditions over the last year." (Ex. A, p. 82.)**

D/R neither cite nor produce any studies that were done in 2008 that examined the effects of media coverage on public awareness of algae conditions.

These opinions, based on speculation instead of sound methodology, should be excluded.

E. D/R's Opinions on Their Scope Test Should Be Excluded as Unreliable

In Chapter 4.4.1, D/R argue that the CV survey does not pass a revised scope test they developed. (Ex. A, p. 70.) The scope test they developed, and all opinions based thereon, should be precluded because their scope test is not supported by any peer-reviewed literature and is not conducted by way of a proper statistical test. (Ex. C, Kanninen Decl. ¶ 51.) Under the D/R revised scope test, after artificially reducing the sample size used in the CV Report for the base instrument, they estimated WTP for the base and scope instruments and compared the estimated confidence intervals. (*Id.* ¶ 52.) Comparing confidence intervals, however, is not a proper statistical test. (*Id.* ¶ 53; Desvousges Depo Tr. at 134:12 ("No, it's not a statistical test.").) The proper test would be a "t-test" for the comparison of two sample means, which when estimated, results in the rejection of the null hypothesis, and the scope test is passed, as explained in Ex. C, ¶¶ 54-55. This directly contradicts their claim. (*Id.* ¶ 55.) Because D/R do not use a statistical test in their analysis, their opinions relating to their scope test were not reached using sound methodology and must be excluded under Rule 702 and *Daubert*.

F. D/R's Opinions on the Statistical and Econometric Analyses in the CV Report Are Fundamentally Flawed and Should Be Excluded

In Chapter 5, D/R "examine the statistical and econometric analyses presented in the Stratus CV Report." (Ex. A, p. 91.) Because their analysis is so riddled with errors and flawed methodology, the opinions therein should be excluded as unreliable.

1. *D/R's Opinions Relating To the Turnbull and ABERS Estimators Should Be Excluded as Unreliable*

First, D/R claim that the CV Report's willingness-to-pay ("WTP") estimate "derived using the ABERS estimator adopted by Stratus is greater than the estimated WTP using the Turnbull estimator."[16] (Ex. A, p. 93.) D/R claim that the Turnbull estimator is "more conservative" and "more appropriate." (*Id.*, pp. 91, 105.) However, because they used a flawed mathematical equation, all of D/R's Turnbull calculations in Chapter 5 are incorrect. (Ex. C, Kanninen Decl. ¶ 19.) In fact, when calculated correctly, the Turnbull and ABERS estimators yield identical results for the type of data considered in the CV Report. (*Id.* ¶ 18.) Because D/R's opinions in Chapter 5 are almost entirely premised on their erroneous Turnbull estimations, Chapter 5 and all related testimony should be excluded as unreliable.

As Rausser admits, the purpose of an estimator is to achieve "maximum likelihood estimation," (Ex. I, Rausser Depo Tr. at 146:4-7), which is the standard approach to estimation in the field of economics. (Ex. C, Kanninen Decl. ¶ 25; Ex. U, Hanemann Decl. ¶ 9.) The question is which estimator (or estimators) achieves "maximum likelihood estimation" with the type of data at issue (i.e., "single-bounded" data, as here, with "yes" and "no" votes in response to a single-vote question (*id.* ¶ 18); or "double-bounded" data, not at issue here). When estimated correctly, the Turnbull and ABERS estimators are identical for single-bounded data, the type of

[16] "ABERS" is the acronym for the authors of Miriam Ayer, et al., *An Empirical Distribution Function for Sampling with Incomplete Information*, 26 Annals Mathematical Stat. 641-47 (1955) (Ex. Y), which describes the ABERS estimator. (Ex. C, Kanninen Decl. ¶ 21.)

data considered in the CV Report. (Ex. C, ¶ 24; Ex. U, ¶¶ 16, 24.)

The sole reference D/R provide for their use of the Turnbull estimator is a non-peer-reviewed book chapter, Haab-McConnell 2002.[17] (Ex. A, pp. 93 n.59, 116; *see* Ex. C, Kanninen Decl. ¶ 30.) Although Rausser described this book chapter as a "perfect substitute" for the peer-reviewed literature, Ex. I at 129:5-23, he apparently only considered a few pages, as only an excerpt of the chapter was produced in his considered materials. (*See* Ex. V.)

The excerpt produced in Rausser's considered materials does not contain Haab-McConnell's complete discussion of the Turnbull approach in their book. In the portion of the chapter omitted from Rausser's considered materials, Haab and McConnell present a mathematical proof regarding the Turnbull estimator. (Ex. C, Kanninen Decl. ¶¶ 30-31.) In their proof, however, they commit a number of mathematical errors. (*See* Ex. C, Kanninen Decl. ¶¶ 31-34; Ex. U, Hanemann Decl. ¶¶ 17, 20.) Haab-McConnell (2002)'s application of the Turnbull estimator is inherently flawed and is not found in the peer-reviewed statistical or econometric literature.[18] (Ex. U, ¶ 17.) When these errors were brought to Professor Haab's attention, (*id.*, ¶ 21), he responded as follows:

> I am in agreement that there is a discrepancy between Ted McConnell and my 1997 Journal of Environmental Economics and Management article and our 2002 book treatment of the Turnbull estimator for a lower bound on expected willingness to pay. Further, I'm in agreement that this difference is not trivial and leads to differences in the calculation of the lower bound on willingness to pay.

(6/15/09 email from T. Haab to M. Hanemann, attached to Ex. U.) Professor Haab agrees that the 2002 book treatment "is not mathematically correct" and that "the Turnbull lower bound on expected willingness to pay treatment in [the 1997 article] is correct and consistent with the

[17] Timothy C. Haab & Kenneth E. McConnell, Valuing Environmental and Natural Resources (2002) [hereinafter "Haab-McConnell (2002)"].

[18] Indeed, Rausser could not identify any application in the literature where a statistician doing a non-parametric maximum likelihood estimation of single-bounded, data, as here, used the algorithm applied in Haab-McConnell (2002). (Ex. I at 144:17-145:11.)

original treatment as explained by Ayers, Morgan and Turnbull." (*Id.*; Ex. U, ¶ 23.)

In rendering his opinions on the Turnbull and ABERS estimators in this case, Rausser did not consult the *peer-reviewed* literature on these estimators, which includes, at a minimum, the 1974 and 1976 papers by Turnbull setting forth the Turnbull estimator,[19] as well as Ayer, supra note 16 (Ex. Y), which is the basis of the ABERS estimator. (*See* Ex. C, Kanninen Decl. ¶¶ 20-21.) D/R considered none of this scholarship, as none appears in their considered materials. (*Id.* ¶ 22; Ex. I, Rausser Depo Tr. at 128:18-130:3)

Relying on the flawed methodology of Haab-McConnell (2002), D/R incorrectly claim that the Turnbull estimator of mean WTP is different from the ABERS estimator when the data collected do not exhibit "monotonicity" across bids.[20] (Ex. C, Kanninen Decl. ¶ 26.) In the case of the data presented in the CV Report, there is a modest non-monotonicity between the bids of $80 (where respondents voted "yes" 60.2% of the time) and $125 (where respondents voted "yes" 61.5% of the time). (*Id.*) When non-monotonicity occurs in the data, the ABERS approach is to pool the two percentages and apply the average (weighted by their respective sample sizes) to both bids. (*Id.* ¶ 27.) Relying on the flawed Haab-McConnell (2002) derivation, however, D/R claim that, in this context, the Turnbull estimator only applies the pooled result to the lower bid. (*Id.* ¶ 28.) Thus, instead of applying an averaged response of 60.9% to both the $80 and $125 bids, D/R apply the 60.9% to the $80 bid, dropping the $125 bid. (*Id.* ¶¶ 27-28.) This approach is not supported by any peer-reviewed statistical, theoretical,

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[19] Bruce W. Turnbull, *Nonparametric Estimation of a Survivorship Function with Doubly Censored Data*, 69 J. Am. Stat. Ass'n 169-73 (1974) [hereinafter "Turnbull (1974)"] (Ex. W); Bruce W. Turnbull, *The Empirical Distribution Function with Arbitrarily Grouped, Censored and Truncated Data*, 38 J. Royal Stat. Soc'y 290-95 (1976). (Ex. X.)

[20] Monotonicity, in the context of the type of data presented in the CV Report, means that votes in favor of a program should consistently go in one direction – i.e., down, by economic theory – as bids (the cost to households of the program) increase. (Ex. C, ¶ 26.) Due to random sampling error, pure monotonicity is not always manifested in collected data. (*Id.*)

or mathematical derivation. (*Id.*)

Indeed, the peer-reviewed literature demonstrates that the ABERS estimator is the "maximum likelihood estimator" for the type of data that the CV Study collected (i.e., single-bounded data, with "yes" and "no" votes in response to a single vote question).[21] (Ex. U, Hanemann Decl. ¶ 10.) Even Turnbull (1974) (Ex. W) explicitly states that the ABERS estimator is the maximum likelihood estimator when the data are single-bounded, i.e., the type of data the CV Study collected. (Ex. C, ¶ 25, Turnbull (1974), p. 170 ("Ayer et al. . . . have derived explicit expressions for the maximum likelihood estimates" for single-bounded data).) Furthermore, in their 1997 *peer-reviewed article* on this subject,[22] Haab and McConnell apply the ABERS estimator, even using the same dataset used in their 2002 book. (Ex. C, Kanninen Decl. ¶ 35 (citing Turnbull, supra note 22); Ex. U, Hanemann Decl. ¶¶ 19, 24.) The foregoing demonstrates that ***the Turnbull and ABERS approaches are identical when the data are single-bounded, as here.*** (Ex. C, Kanninen Decl. ¶¶ 34, 37; Ex. U, Hanemann Decl. ¶¶ 16, 19, 23-24.) This fact – clearly stated and supported in the peer-reviewed literature – contradicts the claims of D/R that their "Turnbull" estimator is more conservative than the ABERS. (*Id.* ¶ 37.)

In sum, D/R's estimation of the Turnbull estimator is not found in any peer-reviewed literature and is based on the flawed mathematical derivation in the non-peer-reviewed book chapter in Haab-McConnell (2002). D/R's opinion that the Turnbull estimator is a more conservative and more appropriate approach to estimating WTP than the ABERS estimator is, therefore, without any reliable basis or grounding in sound methodology. Because all the

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[21] The literature also demonstrates that the Turnbull estimator is an extension of the ABERS estimator where data are double-bounded, which is not the case here. (Ex. C, Kanninen Decl. ¶ 24.) The Turnbull *extension* is therefore irrelevant to the type of data here. (*Id.*)

[22] Timothy C. Haab & Kenneth E. McConnell, *Referendum Models and Negative Willingness to Pay: Alternative Solutions*, 32 J. Envtl. Econ. & Mgmt. 251-70 (1997).

Turnbull estimates in the D/R Report are wrongly calculated, such estimates, resulting opinions, and related testimony from Chapter 5 and corresponding appendices should be excluded under Rule 702 and *Daubert*.[23]

2. *D/R's Opinions on the Elasticity of the WTP Measure in the CV Report Should Be Excluded*

In Chapter 5.3, D/R also estimate income and price elasticities of WTP and claim that their results "raise serious questions about the validity of the Stratus CV study." (Ex. A, p. 103.) D/R's analysis of elasticities is unreliable for several reasons. *First*, D/R commit several errors in their estimation of income and price elasticities (*Id.* p. 102), reflecting the failure to apply a sound methodology.[24] (Ex. C, Kanninen Decl. ¶ 38.) They include:

- D/R estimate elasticities by substituting the change in voting "yes" for the quantity demanded. Although they claim that "[t]his technique has been repeatedly recognized in the literature," (Ex. A, p. 99 n.64), D/R list only two supporting citations, neither of which deals with estimating elasticities. (Ex. C, Kanninen Decl. ¶ 39.)

- All of the elasticities are incorrectly reported in Table 5.5 (Ex. A, p. 102). (Ex. C, ¶¶ 40-41 (explaining error).) (To test the results in Table 5.5, D/R's computer code was re-run using the same dataset D/R used, revealing these errors.) In short, all three sets of results are presented in incorrect, reverse order in Table 5.5. (*Id*.)

- Although Table 5.5 contains a column labeled "mean income," Rausser testified that this column instead represents the mid-point of the income range. (Ex. I at 136:12-20.) The mid-point is not the same as the mean.

- D/R do not report the sample sizes of the income groups, which are as small as 115 observations for the case of sextiles. (Ex. C, ¶ 43.) With sample sizes this small, D/R's models and estimated elasticities are statistically unreliable. (*Id.*)

- D/R commit a fundamental analytical error by simultaneously disaggregating the sample into four to six sub-groups ranked by income and then re-estimating the Stratus logit model using log-income as a variable *within each sub-group*. (Ex. C,

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[23] Against this background, it appears D/R lack the expertise to testify on estimators.

[24] "In economics, elasticity is measured as the percentage change in quantity demanded with respect to a percentage change in price. However, with WTP data, there is generally no continuous variable available to represent the quantity demanded in the standard elasticity formula." (Ex. C, Kanninen Decl. ¶ 39.)

> ¶ 44 (explaining error).) Because this reduces income variation within sub-groups, the income coefficient in D/R's model will not be a statistically reliable indicator of how income affects voting and, therefore, the coefficient cannot be used to estimate a reliable income elasticity. (*Id.*)

Second, because D/R fail to report confidence intervals or standard errors on their calculated elasticities, there is no evidence that their results are statistically significant, nor is there any way to compare results by way of statistical testing. (Ex. C, Kanninen Decl. ¶¶ 45-47.) *Third*, D/R fail to understand the coding of a key variable in their analysis, the income variable.[25] (*Id.* ¶¶ 38, 48-49.) *Fourth*, D/R dropped a large number (254) of observations from their analysis due to their misunderstanding of the coding and their failure to impute income for the missing values. (*Id.* ¶¶ 38, 50.) Dropping this many observations can result in an unrepresentative sample with biased results and renders the results less precise statistically. (*Id.*)

Finally, D/R fail to support their approaches to estimating elasticities with relevant references to the peer-reviewed literature. (*Id.* ¶ 38.) Regarding their claim that the result of their elasticity calculations "is inconsistent with expectations based upon the extensive literature previously described," their reference to extensive literature is misleading, as they cite *no* literature relating to the price elasticity of demand for a commodity such as improvement in rivers and lake. (*See* Ex. U, Hanemann Decl. ¶ 30.) They cite one reference that relates to the price elasticity of demand (*see* Ex. A, p. 98),[26] which is a review of the literature on the price elasticity of the *agricultural* demand for *irrigation water* as an *input* to agricultural production, which bears no relation to the household demand for water as a final good, let alone the household WTP for improvement in rivers and lakes. (Ex. U, ¶ 30.) D/R's other citations (pp.

[25] While codes at or above a value of 99999999998 were used to identify respondents who either did not know their incomes or refused to answer the income question, Rausser incorrectly stated that "[t]here was some coding from Stratus that indicated that income levels were above 99 billion or whatever it is." (Ex. C, ¶¶ 48-49; Ex. I at 137:9-12.)

[26] Susanne M. Scheierling et al., *Irrigation Water Demand: A Meta-Analysis of Price Elasticities*, 42 Water Resources Res. W01411 (2006).

98-99) are to estimates of the *income* elasticity of WTP, not price elasticity. (*Id.* ¶ 31.) Moreover, the studies cited employ *parametric* models of the WTP distribution, not non-parametric models like that in the CV Report, making them non-comparable. (*Id.*)

In sum, D/R's opinions regarding elasticities of WTP reflect a lack of understanding and/or experience with non-parametric estimation and are not grounded in a sound scientific methodology. (*Id.* ¶ 35.) They should be excluded under Rule 702 and *Daubert*.

Based on D/R's failures to exercise sound methodology in critiquing the CV Study, as described above, all of D/R's opinions in Chapters 4 and 5 should be excluded as unreliable.

G. D/R's Opinions re: the Past Damages Report Should Be Excluded

D/R's opinions relating to the "benefits transfer" approach in the Past Damages Report should be excluded because they are not based on sound methodology. (Ex. A, pp. 121-26.) D/R misrepresent and misstate the relevant literature, stating that benefits transfer is only "a spatial concept" and that "applying values backwards in time is not reliable." (*Id.*, pp. 121-122.) However, the science, practices and their own work show that benefits transfer is a temporal concept as well as spatial. As stated in the V.K. Smith (1999) paper, which D/R cite (p. 127):

> Benefit transfer is the practice of adapting available estimates of the economic value for a change in environmental quality (or quantity) to evaluate a proposed, policy induced, change in the same or a "similar" resource. . . . [T]he analyst is typically taking the results from one or more existing studies (defined in terms of their ***time frame***, the location, the environmental resource, or quality change, and the affected population), and transferring them to a different context that is relevant for a policy being evaluated.

V. Kerry Smith et al., *Benefit Transfer as Preference Calibration* 2 (Resources for the Future Discussion Paper No. 99-36, 1999) (emphasis added). By definition, every benefits transfer conducted is a transfer through time. Studies done in one place ***and time*** are transferred to another situation. These transfers can occur years, even decades, later. For example, one of the most comprehensive benefits transfer applications ever conducted in the U.S. was the EPA's

retrospective study on the benefits of the Clean Air Act. *See* U.S. EPA, *The Benefits and Costs of the Clean Air Act, 1970 to 1990* (1997), *available at* http://www.epa.gov/air/sect812/copy.html. In that peer-reviewed study, the researchers transferred through time benefit values developed between 1979 and 1996 to estimate the benefits of reducing health related injuries between 1970 and 1990. *Id.* at 43; *see generally id*. at app. i.

D/R's opinions and statements in Chapter 7 are misleading to the court, particularly as Desvousges' own work has applied the very method he criticizes. While D/R states: "To our knowledge, the literature on benefits transfer contains no references to studies that extrapolate damages backward in time" (Ex. A, p. 122), one of Desvousges' own studies, used in the context of the settlement of natural resources damages claims, undertook this type of transfer.[27] In that study, Desvousges transferred recreational fishing benefits estimated in the year 1998 to the past, as far back as 1981 (27 years) and as far forward in the future as to 2050. There, he states:

> The 1998 survey data provide a snapshot of angling activity. The models provide estimates of the losses for the anglers in the sample during the survey period. To estimate total losses, the survey results are aggregated to reflect the losses of all anglers from 1981, the first full year after the passage of [CERCLA], until the advisories are removed. The gains will occur from the time the restoration projects are built until they fully depreciate in 50 years.

(Id., pp. 15-16.) Of course, 1998 to 1981 is a 27-year period in the past.

In sum, D/R's opinions relating to the benefits transfer methodology and literature are not reliable and should be excluded under Rule 702 and *Daubert*.

V. Conclusion

The State requests that the Court enter an order *in limine* excluding D/R's opinions in Chapters 2, 3, 4, 5 and 7 and corresponding appendices in their Report and any related testimony.

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[27] William H. Desvousges et al., *Lower Fox River and Bay of Green Bay: Assessment of Potential Recreational Losses and Restoration Offsets* (2000). (Excerpts attached at Ex. Z.)

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I hereby certify that on this 19th day of June, 2009, I electronically transmitted the above and foregoing pleading to the Clerk of the Court using the ECF System for filing and a transmittal of a Notice of Electronic Filing to the following ECF registrants:

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